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Interactive comment on “Velocity structure and the role of fluids in the West Bohemia Seismic Zone” by C. Alexandrakis et al.

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General comments: I am not an expert on either double-difference tomography or West Bohemia seismotectonics, but have been in contact with both topics through colleagues. However, this means that I cannot place the paper at hand in the current state-of-the-art. To my knowledge, it is the first tomography performed for that area (in such detail).

Nevertheless, the paper is written clearly and concisely. I had no difficulties in following and understanding the explanations. The synthetic and resolution tests seem to be well chosen and render the results of the study trustworthy. The interpretation of the results is largely coherent, but would gain from a more detailed explanation.

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Please don't feel dispirited by the numerous comments below, most of it is really small stuff and can be corrected easily.

Specific comments:

1. Introduction, p. 513, l. 24: The “variety of input parameters” for WAM, which is used for tomography: what input parameters are available?
2. Data, p. 514, l. 6: What exactly do you mean by “strongest swarm”? The swarm with the most earthquakes? The swarm exhibiting the largest magnitudes?
3. Data, p. 514, l. 12: What were the selection criteria for the 483 events relocated with HypoDD and why is the number reduced to 473 for the tomography?
4. Methodology, p. 515, l. 25: Why were P-velocities only perturbed in depth, but not laterally?
5. Methodology, p. 515, l. 27: Could you provide a figure (or extend one of the present figures) in order to show the differences in location between FASTHYPO and HypoDD event locations? After all, they seem to be substantial enough to require the usage of both data sets. In case you don't want to repeat a figure from Bouchaala et al. (2013), you may at least cite the maximum difference in locations in metres or a general shift in the spatial pattern of locations or similar.
6. Model resolution, p. 516, l. 14: What do you mean by “an error in the rupture onset time was found”? The tomography results from an analysis of a whole set of earthquakes, not just one specific rupture? I would guess it is rather an error in the station timing in general?
7. Model resolution, p. 516, l. 25: please repeat here, that the vp/vs models show similar structures and values below a certain depth (5 km), especially since the respective figure is located in the supplements.
8. Model resolution, p. 517, l. 10: Synthetic traveltimes are calculated using the WEB-

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NET earthquake locations. . .but your tomography is performed using both the WEB-NET (FASTHYPO) and HypoDD locations, shouldn't you compute the synthetic traveltimes according to both sets of locations as well? Or have tests been only performed using one set of locations? This would be completely okay, since the locations probably do not differ so much, but please state this in the text. Which method/software did you use to compute the synthetic traveltimes?

9. Model resolution, p. 517, l. 24: Why are P and S synthetic traveltimes computed for eight and not four synthetic models? Did you count the S-velocity model extra? I would just count it once (P- and S-velocity changes together), since the S-velocity change depends on the chosen P-velocity and v_p/v_s ratio.

10. Model resolution, p. 518, l. 2: Maybe you should remark on the fact that also the v_p/v_s models only recover the overlying anomaly directly above the earthquake cluster, not laterally.

11. Model resolution, p. 518, l. 6/7: Is there a “neutral” node between two neighbouring cells of the checkerboard? Do the horizontal nodes have the same intermodal distance in N-S and E-W direction?

12. Results, p. 519, l. 21: Here, you talk about 0.5 km depth intervals for the computation of the average values, whereas in the caption of Fig. 6, you describe 1 km depth intervals.

13. Results, p. 519, l. 20: Can you comment in more detail the comparison of average velocities from the computations to the ones from the regional model? E.g. is there an explanation why P- and S-velocities are universally higher than in the Málek-model?

14. Results, p. 520, l. 26: I don't understand what you mean by “a cross section parallel to the 9HR/91 profile is limited to the northwest by the station geometry”.

15. Results, p. 521, l.2-9: It's a bit simplistic to say that the projected earthquake foci are situated within a lower density granite body, isn't it? When looking at Fig. 7, they

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seem to be situated on the transition between granite (color no. 1) and the mysterious striped layer (no. 4). Even if both these layers represent a similar lithographic unit, still the hypocenters of the 1994 swarm are situated – along with the majority of earthquakes occurring in the time period 1991-1994 – in the metasediments/metabasites layer (the layer, which is described as cap rock resisting to fracturing).

16. Results, p. 521, l. 16: It should be possible to differentiate between slip along preexisting fractures and the formation of new fractures by performing moment tensor inversions of the events within the 2008 swarm. Especially the occurrence of tensile fractures would back-up the hypothesis of high pore pressure and thus high v_p/v_s ratio within the focal zone. Has this been tried? (Not by you, but others!)

17. Results, p. 521, l. 17/18: I do not fully understand your reasoning for the cycle. If preexisting fractures slip or new fractures form in the granite, then the permeability is enhanced within this layer and not the overlaying layer. Thus, this enhanced permeability does not help the migration of fluids toward the surface other than forming a new fracture within the overlaying layer as well. In that case, shouldn't you observe seismicity moving upwards to the surface? Maybe also lateral movement would be a solution...but why and where would it stop? If I recall correctly, migration of event locations (upwards/sideways) is observed for all the swarms in this area. Would it be an idea to overlay the event locations colored by time onto the geological plot? Maybe not just for this swarm, but the others as well? Or are they already too far out of the model range? In addition: according to your description, it is rather a cycle of pressure increase and pressure decrease (by loss of fluid) than a cycle of pressure increase and stress release. Or were you referring to the stress release by slip of preexisting/new fractures? In that case, it is not yet a cycle, since this will not reduce the pressure? I'm not saying that the idea is not viable, but it should be fortified and explained in more detail.

18. Results, p. 522, l. 4: "Earthquakes only occur below 7 km depth". . . maybe replace by "earthquakes in this swarm only occur below 7 km depth"? The red circles in Fig.

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7 occur all over the place. Or is it always the swarm earthquakes, which stay below 7 km, whereas “single” earthquakes may also take place at lower depths? Probably not, since the hypocenter of the 1994 swarm is also plotted at lower depth, in fact within the less brittle material. Please elaborate on that (not necessarily in the conclusions, rather when explaining Fig. 7).

19. In general, is it possible to VERY BRIEFLY compare the swarm characteristics (main differences/conformities) of this swarm to preceding/subsequent swarms?

20. Fig. 3: In the model, where the region around the earthquakes has been perturbed by a change in P-velocity, why is the negative anomaly (a + b, bottom row) so much better resolved than the positive anomaly? Intuitively, I would have expected a very similar shape of the P-velocity perturbations.

Technical comments:

Since I am no native English speaker, I trust the decisions of the first author regarding the following points:.

1. Abstract, p. 511, line 5: Can you say an “earthquake swarm has principal faults and tectonic stress”? I would put it more precise (admittedly more laborious) saying that “characteristics important for the understanding of the earthquake swarm have been analyzed” or something like that.

2. Introduction, p. 513, l. 23: “a suite of tomography models are calculated”, please correct to “a suite of tomography models is calculated”.

3. Methodology, p. 515, l. 11: For the description of the velocity model by Málek et al. (2001), please refer to Fig. 6, where it is displayed.

4. Methodology, p. 515, l. 28: The full stop is missing between “WAM calculation” and “We also calculated”.

5. Model resolution, p. 17, l. 6/7: if the v_p/v_s ratio is constant and the P-velocities are

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disturbed, the S-velocities are disturbed at the same time, correct? Please indicate so in the text.

6. Model resolution, p. 518, l. 13: “The checkerboard WAM is calculated using the same model parameterizations as the observed data WAM”: please change to “The checkerboard WAM is calculated using the same model parameterizations as for the observed data WAM”.

7. Results, p. 519, l. 2: Is it correct to say “compressional/shear velocities” instead “compressional/shear wave velocities”? If not, this applies to other parts of the paper as well.

8. Results, p. 519, l. 10: comma missing in citation “Spicak and Horalek, 2001”.

9. Results, p. 519, l. 16: Isn’t it more correct to say “layer corresponds to” instead of “layer corresponds with”? In addition, the depth of the layer corresponds to the depth of the shallowest relocated earthquake (not the layer to the earthquake).

10. Results, p. 519, l. 20/21: if you remove “calculated” from “average calculated values” (l. 20), you avoid the word repetition of “calculated” in l. 21.

11. Results, p. 519, l. 23/24: sloppy expressions: “low v_p/v_s ratio layer”, “high focal zone values”. Please replace by a more precise description, e.g. “the layer exhibiting a low v_p/v_s ratio”, “the focal zone featuring high v_p/v_s ratios”.

12. Results, p. 520, l. 4: “it” increases. It is not clear, what “it” refers to, the P-velocity itself or the v_p/v_s ratio.

13. Results, p. 520, l. 21: “the depth increases [] and has its root”. . . I guess it is rather the Fichtelgebirge, which has its root along the MLF?

14. Results, p. 521, l. 1: “metamorphic”, not “metamophic”

15. References, p. 524, l. 22, Husen and Kissling (2001): “Chile”, not “Chili”?

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16. References, p. 524, l. 27: Please remove the double “Ee” from “Eeberhart-Phillips”.
17. Fig. 1: The writing on the map seems to be very small, especially the names of towns. Is it in addition possible to enlarge the symbols plotted on the map?
18. Fig. 2: Titles and number associated with the colour bars are far too small in printing.
19. Fig. 4: Titles and number associated with the colour bars are far too small in printing. Why does the colouring appear more saturated in (a) bottom than in (a) top? The variations should be of the same amplitude? Please note in the caption that the top row shows N-S profiles, whereas the bottom row shows E-W profiles. Maybe it would be helpful to overlay the event locations on these plots as well? Or do they cover too much of the pattern?
20. Fig. 7: Again, the writing on the left figure is too small to be readable. You would first refer to the left figure, before referring to the right figure. There is a verb missing in the 2nd sentence: “The vp/vs model profile is [] and through the focal zone”. Replace by e.g. “. . .and passes through the focal zone”. Further, replace “the low vp/vs layer” by the more precise expression “the layer exhibiting low vp/vs values” (or something similar). The black circle does not show the swarm, but the location of the swarm. Please explain also the meaning of red circles and yellow star within the caption. To me, there is no such thing as a “hypocenter” of a swarm. . .maybe a centre/centroid/focus of the hypocenters of earthquakes within the swarm? The figure on the left, was it plotted by you? If yes, please consider choosing more appropriate symbols (e.g. smaller dots representing single earthquakes, the same symbol indicating the centroid location of the 1994 and 2008 swarm). If not, please indicate from which paper you took the figure. From the legend, it is not clear what the meaning of the “striped layer” (no. 4 in the original legend) is, although that would be quite crucial for the discussion, since the 2008 swarm seems to be situated at the transition between granites and this material.
21. Why is the title of your supplement document: “How the western frontiers were

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won with the help of geophysics”?

22. Supplement, Fig. 1-3: please correct “Supplimentary Figure” with “Supplementary Figure”

23. Supplement, Fig.1b: The coloring is too weak. Can you restrict the color bar to smaller numbers?

24. Supplement, Fig. 1: Would you really say “localized P-velocity increase”? Not “local P-velocity increase”?

25. Supplement, Fig. 2: It is not so critical, that the numbers on x-and y-axis are printed small, since the range is the same as in e.g. Fig. 2. But the numbers on the axes of the velocity model plot are too small as well, please enhance those. Not to mention the numbers on the colour bars, which are unreadable (maybe change placing of the figures in order to gain more space for the single figures?).

Interactive comment on Solid Earth Discuss., 6, 511, 2014.

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6, C450–C457, 2014

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